Comment on amt-2022-236

Anonymous Referee #2
Referee comment on "Drone-based meteorological observations up to the tropopause" by Konrad Benedikt Bärfuss et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-236-RC2, 2022

The Authors would like to thank the referee for taking the time to review our work – which is highly appreciated in such busy times.

Preface:
Despite the valuable comments and suggestions, we had to discern that the focus of our study might have been misinterpreted here - possibly caused by the title and the lack of clear statements regarding the focus. We now state clearer, that the aim of the study is to provide an introduction to Aircraft Based Observations with small UAS and to present first meteorological observations of a self-powered small UAS using a simplistic sensor setup to demonstrate today's capabilities of small UAS as a "carrier platform".

Let me begin by saying that I am a strong supporter of the utility and benefit of automated aircraft reports to fill the time and space gaps left between other in situ observing systems. I therefore read the article entitled "Drone-based meteorological observations up to the tropopause" by Bärfuss, Schmithüsen and Lampert with interest. Unfortunately, I came away from it feeling disappointed and uninformed. As it stands, the text reads much more like a "Concept Study" or perhaps "A Report on Preliminary Project Activities" than a scientifically supportable journal article and needs to be substantially revised and enhanced before it can be considered for publication. Some of my reasoning for this decision is summarized below.

Firstly, the authors have dedicated nearly 1/3 of the article to the Introduction and background materials. And even then, they have not included any discussion of problems that other authors have discovered with automated reports from commercial aircraft that should provide a far more stable platform for collecting data and providing representative measurements or the atmosphere without being affected by artifacts related to aircraft stability. For example, it has been documented that AMDAR observations from longer range aircraft provide more accurate wind reports than TAMDAR reports obtained from smaller/lighter regional jets, whose wind reports are much more susceptible to the effects of turbulence than the larger/heavier aircraft. See recent papers by Wagner et al. on this topic. The excessive presentation of background material also detracts from the limited analysis of the flight results.

You might refer to Moninger et al. 2010 [1], which states: "The lower quality of the wind data from TAMDAR is likely due to the less accurate heading information provided to TAMDAR by the Saab-340b avionics system. Accurate heading information is required for the wind calculation, and the Saab heading sensor is magnetic and known to be less accurate than the heading sensors commonly used on large jets."
Literature in general links accuracy to the avionics/navigation systems used to calculate wind in AMDAR/TAMDAR – the accuracy is not linked to the size of the aircraft, which is also reflected by several small research aircraft flying at low altitude, which provide the wind vector with high quality (e.g. D-IBUF / D-ILAB / HALO D-ADLR / Polar 5 C-GAWI / Falcon D-CMET etc.)

Using the simple (T)AMDAR approach to estimate wind (pure vector difference, see [2]), no rotational effects are included, and observations are taken as invalid above a certain aircraft roll threshold. In addition, the side slip angle and the angle of attack is assumed to be constantly zero.

Doing so, turbulence definitely folds into measurements, but only because of the algorithm and simplifications applied – depending on e.g. the averaging time span and aircraft specific motion dynamics. A discussion of such peculiarities depending on the algorithm would be outside the scope of the article. Nevertheless, we included more details about how wind measurements were derived and finally decided to include a more detailed data analysis of the measurements as a case study using a simplistic sensor setup on a small UAS.

A fundamental issue of terminology is also scattered throughout the paper related to the phrase "Uncrewed Aerial Systems" (or UAS). According to online references on drones, "a UAS or

“Unmanned Aircraft Systems” includes not only the UAV or Drone but also the person on the ground controlling the flight and the system in place that connects both. Basically, the UAV is a component of the UAS, since it refers to only the vehicle itself.” As such, why is yet another (possibly conflicting) terminology being added the already excessive meteorological acronym list?

We are well aware of the discussion on terminology. Around 10 years ago, the term unmanned aerial vehicle (UAV) was the standard term to describe the unmanned aircraft, while the term unmanned aerial system (UAS) referred to the whole system, including in particular the ground-control station. In those times, the term drone was mostly used for military unmanned systems. To overcome the different terms, the term remotely piloted aircraft system (RPAS) got more familiar. For gender reasons, the term UAS was spelled out “Uncrewed aerial systems” (one might refer to the acronym “uncrewed spacecraft”) to keep the well-known acronym. Today there is a tendency of using the term drone to avoid gender conflicts. For simplicity, we now use the expression “drone” throughout the manuscript.

When compared to other sections of the paper, the introduction length (143 lines) seems to be excessive. For example, the important information about instrument accuracy in the section titled Sensor Package uses only 13 lines (and includes a Table that is never referenced in the text and contains an undefined term “prospective”). For other aircraft observing systems, detailed air tunnel tests were performed with the full system to determine whether the installation approaches would degrade the observations further. The reader can only assume that no such tests were done of the LUCA installation approach. The only vague reference notes that some temperature observations were affected by “heat transfer inside the fuselage to the sensor head”. The authors also assume that the HMP110 sensor package will work as well on a drone moving through the atmosphere (at a speed not clearly specified in the paper) as it does on a radiosonde package suspended from a balloon moving with the atmospheric flow, not through it. This should not be considered a given.

As not everyone interested in the article is aware of the data base for numerical simulations, we disagree on the introduction length. The introduction is of high importance to motivate the development of our system, as there is currently no such tool available to perform this kind of measurements.

We totally agree that sensor selection and installation are very important. We now point out in the manuscript more clearly that we have used and characterized the same types of sensors on drones flying at similar air speed, and methods how to take care of limited sensor response time. However, the purpose of the manuscript is to present the novel development of the drone covering an altitude up to 10 km.

Indeed wind tunnel measurements may provide insights to degrading effects of the installation approach, but one has to be aware, that these tests are not replacing in-flight calibration of e.g. the wind measurements.

For example, when the TAMDAR system was developed, questions regarding biases produced by instrument lag using capacitive humidity sensors flying through the atmosphere were sufficient to change the instrument design to use dual sensors whose reports were averaged.

While the sensor lag of capacitive sensors is not eliminated by deploying two identical sensors (to provide redundancy as used in TAMDAR, see e.g. Mulally 2009 [3]), we agree that the complementary use of different sensor principles is of high advantage. Sensor readings might have quite complex transfer functions between the atmospheric state and the observation quantity. On current research aircraft, the approach is to combine sensors of different measurement principles with different advantages, e.g. a stable, high accuracy sensor with a sensor of drifting signal, but high resolution response time, by complementary filtering (high pass for the fast sensor, low pass for the slow sensor with suitable cutoff frequency).

I also expected to see some discussion about how wind measurements were derived from the aircraft and how they were affected by turbulence, especially near the level of the jet stream. Unlike other proven aircraft observing systems which attain speeds much larger than the ambient wind in the upper troposphere, the drone discussed here is likely to be flying at speeds much less than the atmospheric flow. As such, much of the wind vector determination will be dominated by change in geographical location.

Based on your comment, we decided to include more information about the wind derivation method, which enables further discussion whether wind speed and turbulence impact the accuracy of the wind vector calculation. In general, low air speed (flight speed) increases the accuracy of the wind measurements, as the vector difference (between the air flow vector and the inertial motion vector) benefits from small vector length assuming nonzero angular errors (here, the vectors are assumed to be defined by angles and lengths). The wind vector is not derived from changes in geographical locations, as it is the case for the classical radiosonde.

Without this documentation [or clear knowledge of the system outputs (e.g., GPS locations, drone speed and direction, maximum flight duration, maximum distance from takeoff site to tropopause observation, residence time at tropopause elevation, etc.) that are used to determine the meteorological parameters], it is difficult to assess either the utility of the drone observations or the source of differences between the drone observations and other sources.

We now state more clearly in the manuscript:
“The drone measurements can take place from any location, up to the altitude of 10 km, if flight permission is obtained. It is completely independent of additional infrastructure, like an airport, or the availability of helium. The drone only requires a cylindrical air space with a radius of about 11 km for the whole mission. The drone ascends and descends at a vertical speed of 10 m/s and a horizontal speed component of 100 km/h. Therefore, the mission up to 10 km altitude and back to the landing site takes in total 33 min. The data is available by remote transfer with a temporal resolution of 1 Hz. The full data set with a resolution of up to 100 Hz can be downloaded after landing, and is processed automatically for upload in GTS.” During the finalization of the revised manuscript, the text might change slightly.

I would also like to have seen evidence that any object that is 2 m in length and 5-6 kg in mass will not be ‘tossed around’ in the jet stream. This would have to have a substantial impact of some wind observations, which would than need to be eliminated using specialized quality control.

The drone was designed for an air speed of 100 km/h, and it was shown that it is capable of handling this wind speed. As mentioned above, there is no particular impact on wind observations.

Similarly, since the authors state that the temperature reading may be affected by heat generated within the drone itself, is there a method of determining which readings are likely to be contaminated as a function of whether the drone is flying into the wind (in which case the heat generated by the drone should be moved away from the rear of the aircraft and away from the nose mounted sensors) or with a tailwind (in which case the heat could remain in the proximity of the drone and possible affect the sensors).

An aircraft with tailwind is flying through the air at the same true airspeed as it is with headwind, so this is clearly not the case.

Two other sections of the paper provide insufficient support. In the 20 lines of the System Design section, the authors use Figure 1 to provide typical ranges of temperature and wind conditions that were used in the drone design. Panel 1b clearly shows that more than 10% of wind observations at 9-10km exceed 40m/s, yet the instrument is designed to operate in wind speeds < 28m/s. That amounts to a difference in Kinetic Energy of 50%. Significant weather events (e.g., cyclogenesis) are very often associated with very strong jet streaks and substantial ageostrophic circulations that enhance the environment in which the storms form. Accurate knowledge of the sources of Kinetic Energy and its conversion into Available Potential Energy is essential to improve forecasts of these events. If the objective of launching drones is to fill in observations at times when are expected, it is unlikely that the existing design limits of this system will fill that need. This limitation of environments in which wind speeds are < 28m/s makes the use of the phrase “up to the tropopause” inappropriate for inclusion in the title.

This was written misleadingly. The system is designed to climb up to the troposphere even during a constant wind field with FF=28m/s – BUT typically, the wind exceeds 28 m/s only in some altitudes, which enables the UAS to gain ground (fly into the direction where the wind comes from to generate some buffer) before entering the high wind speed.

The aim of developing the drone was to complement regular radiosondes. The focus is not on chasing storms or situations of particularly high wind speed. However, also radiosonde ascents are limited, and launching a radiosonde is only possible up to a surface wind speed of around 20 m/s.

Kinetic Energy is not a suitable concept for describing the developed drone, as only a small amount of Electrical Energy is needed to gain the Kinetic Energy – the mission is dominated by Potential Energy needed to climb up to 10 km, with increasing Frictional Energy (aircraft drag) at high wind speed, when the aircraft is forced to climb at a higher horizontal air speed than 28 m/s.

Also, the authors seem focused on moisture in the middle-upper troposphere, saying that it is particularly important. I'll grant that moisture can have a number of influences at these levels, but the amount of moisture, its vertical structure and horizontal structure are much more important for forecasting weather events from heavy precipitation and flooding to severe storms.

We agree that the amount of moisture, vertical and horizontal structure is of high importance for weather. However, in our study we focus on the potential benefit of additional radiosonde-like data up to the tropopause, whereas prior studies seem to focus more on the boundary layer. Therefore, we analyze in the study at which altitudes there are typically changes at which time scales. In contrast to the upper troposphere, which changes more slowly, and the atmospheric boundary layer with a pronounced diurnal cycle, there is the region in the middle-upper troposphere with irregular, but frequent changes of the meteorological parameters, in particular moisture, which could benefit from additional data.

The section entitled "Potential for covering data gaps with UAS based on atmospheric dynamics" seems mistitled and contains substantial conjecture. Figure 6 provides climatological, not dynamical, information about the rate of change of atmospheric parameters, not the dynamical causes for these changes. It is not new and not particularly relevant to the results presented here, which should be the emphasis of this paper. For example, if the drone system is limited to elevations below ~10km, why to these plots extend up to 35km and how is the reader expected to extract information from them? In addition, phrase like "As one would expect" and "likely results in" are unsubstantiated by the results presented in the paper and detract as conjecture.

As we present small UAS as a potential brick to be part of the Global Observing System, the intention is to show the variability of the atmosphere covering altitude up to the average ceiling of radiosonde measurements. The low variability above the Troposphere/UTLS is also represented by decreased accuracy and spatial resolution breakthrough requirements provided by WMO OSCAR [4].

We moved the section into "Discussion and Conclusion" and substituted "dynamics" by "variability". Soft expressions such as "likely results" were removed, and further information how to interpret the Figure is added.

My biggest issues with the paper, however, relate to the lack of any quantitative assessment of the drone observations or their possible impact on operational weather forecasting.

The focus of the study is to present the new drone system with the capability of reaching an altitude of 10 km. Data gathered by LUCA will be implemented in numerical weather forecast in future studies.

Nevertheless, we decided to analyze the measurements gathered with the simplistic sensor setup as a case study with methods similar to the methods used in Wagner and Peterson 2021 [5].

First, neither Figs 4 nor 5 indicate the units of the wind observations. Are these m/s or knots? Because some LUCA reports in figure 5 show a filled barb on the wind flag, I can only assume that they are knots.

We now added the unit of wind speed in the figure captions.

Figure 5 provides anecdotal information from 2 radiosonde matchups that can only be used subjectively.

The figure shows that radiosondes provide similar data. Of course a direct intercomparison is difficult due to the different methods, and in particular the increasing distance between the two systems. We removed the Panel 5b) and added more information about the comparison within the case study “Assessment of data quality using a simplistic sensor setup on the platform LUCA”

Figure 4, however, indicated that colocation information was available from 5 launches, yet no attempt was made to quantify the differences. In several instances, the authors attribute differences between the two observing systems to time mismatches between the sonde and the drone.

The colocation was redetermined according to the technique used in [5] to intercompare data only within a spatial and temporal limit of 50 km and 30 min. The flights were analyzed in the new subsection “Assessment of data quality using a simplistic sensor setup on the platform LUCA”.

Articles assessing the accuracy of AMDAR observations against a longer series of special radiosonde launches prior to and after multiple AMDAR aircraft ascents/descents showed that the time difference effects were substantially less than other factors.

We agree that this is an important point; however, it is beyond the scope of this article.

When enlarged enough to view the individual wind barbs in Panel 5b, wind speed differences of up to 15 knots are noted at multiple levels around 6km, with erratic directional behavior in the drone observations near 2km and between 4 and 6 km. There is also little evidence in Fig 4 to justify the 2-3 degrees warmer observations near the top of the drone profile in Panel 5b. These differences are crying for detailed quantitative analysis and explanation, but none is available.

We agree that the measurements require a detailed quantitative analysis and explanation in the future. Panel 5b) was removed, as no simultaneously launched radiosonde is available. A quantitative analysis of the data was included as a subsection in “Results”.

However, the main aim of this article is to demonstrate the success of developing a drone capable of flying up to the tropopause. The sensor package that was installed was mainly for demonstration purposes. With more time, and with the experience gained with the drone, a more sophisticated sensor package will be installed. The focus of the study is the carrier system, the drone, and with the preliminary measurement package, the link towards the use in operational meteorology is indicated.

The paper is also devoid of any cost/benefit analysis for the use of a system like this in daily operations. After all, the best system in the world will be of little use if weather services can’t afford to use it. What is the cost of producing one set of ascent/descent reports? How does this compare to existing radiosonde system costs? (FYI, the NWS in the US has done a detailed analysis of this and find the total cost of radiosonde launches to be in the range or $200 to $250 per launch, personnel, instrument, balloon, and gas.)

An attempt to quantify the cost and compare to conventional radiosonde is done in Bärfuss et al., 2022 [6]. We included some sentences about cost in the conclusion/outlook.
As a research article to describe the feasibility of such measurements, cost/benefit analysis have been left out, and commercial vendors would adjust the system according to the market. Taking into account the negative environmental impact of radiosondes (which might be considered substantially different), a cost/benefit analysis relies rather on politics than on training/operating/system cost.

How timely is observation availability, especially if they are to be used on the mesoscale?

In the article we describe the procedure of using the drone. It can be launched any time. Quicklook data are available during the flight. The full data is available after landing, which is 35 min after takeoff. Then the data is then quality checked, transcoded and transferred to the GTS within less than 3 hours after takeoff. Targeted timeliness is 30 min.

Are the observations ‘all weather’, e.g., can the system fly through condition of aircraft icing? If not, the system will become less attractive.

In the article we mention the ability of flying in icing condition and the concept briefly. For some more information, please refer to the technical article on the system LUCA [6].

How quickly can the drone be recharged and sent to another mission?

The turn-around time between landing and the next launch is around 20 min using multiple battery packs and replace them during the ground cycle. This is now stated in the manuscript.

What a training is needed both 1) to launch/retrieve the system and process the data

As the system performs the whole mission automatically, the operator only has to be familiar with the launching and landing mechanism, and can control the performance of the system at the operator station. If problems are detected, e.g. lower climb rate due to icing, the mission can be aborted. Data processing is highly automated. However, this is still a system under development, and not yet a product available on the market.

and 2) maintain the system?

As the maintenance requirements depend on the authorities and the granted permission, we now describe the expected maintenance routines needed in the conclusion/outlook.

How many systems will be needed to fill the needs of the EU?

As the radiosonde net is quite dense in Europe, the system might be more beneficial for other sites. In particular flight permissions may be difficult to obtain in highly populated areas than in other places, e.g. the Antarctic. The system was designed to be operated in the Antarctic environments, as there are less frequent radiosonde launches, and the environmental aspect is of high importance there.

As a result of inadequacies like these throughout the text, it read like a preliminary progress report instead of a quantitatively verified scientific contribution to the literature. Although I support concept presented here, the paper is not ready for publication at this time.

Thank you for your valuable comments and for sharing your perspective. We see that the paper is lacking a data analysis of the measurements (despite the simple sensor setup) as well as strong statements about the focus of the study, which might have distracted you from the main utterance - the feasibility of using small drones up to the troposphere as carrier systems for atmospheric observations.

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